

IN the spring of 1885, at Divonne-les-Bains, I killed a snake, and on cutting it open I found one frog slightly decomposed and another frog apparently dead; the latter recovered in about a quarter of an hour, and hopped away. H. LING ROTH.
32 Prescott Street, Halifax, August 12.

Dogmatism on the Moon and the Weather.

IN a recent little book, "The Story of the Weather," by G. F. Chambers, I have come across one of those *ex cathedra* statements which, I think, illustrate the curious disposition of the mind (even the scientific mind) to circumscribe and limit truth. "No one in his senses," our Meteorological Office is quoted as saying, "can believe in the moon's influence on the weather." Is the matter, then, clear as noonday, or as an axiom of mathematics? Supposing we have, thus far, no proof of such influence, how can we possibly be certain that no such influence exists, or will ever be demonstrated? I happen to be, unfortunately, one of those "lunatics"; but I rather think I am in good company. The author of the book himself, oddly enough, just before approving, apparently, the above dictum, expresses his firm conviction (p. 197) that the full moon scatters clouds! (a point, however, which I cannot say I have studied).

A. B. M.

Rules for Compositors and Readers.

IN the *British Printer* for May and June of last year appears an article under the above heading, by Mr. Horace Hart, Controller, Clarendon Press, Oxford, which, as in my case, may have escaped the notice of some of your readers. On this assumption it would be as well, taking into consideration the importance of the matter to scientific men generally and directors of museums in particular, to ask for the views of others qualified to judge upon the advisability of discarding the use of the digraphs *œ* and *æ* in Greek words written in English characters, in Latin words, and—presumably—in words derived therefrom, such as *Coelenterata* and *Cæsarean*, which, according to Mr. Hart, should not be written, as they usually are, *Coelenterata* and *Cæsarean*. The importance of such a ruling cannot be over-estimated in any museum which desires to teach and not mislead its students—to say nothing of the waste of elaborate labels which the disuse of the digraphs entails, and these considerations must be my excuse for troubling your technical readers for their opinions.

Leicester.

MONTAGU BROWNE.

"ARTIFICIAL FOOD."

UNDER the above title the *Daily Chronicle* of Friday, August 5, prints a telegram from its Vienna correspondent announcing the synthetic preparation, by Dr. Leon Lilienfeld, of albumen having "absolutely the same nourishing qualities as found in that which is obtained from organic beings." Such a synthesis would undoubtedly mark an epoch both in chemistry and physiology, but unfortunately for those who have attached undue importance to Dr. Lilienfeld's announcement, the data given in the sensational telegrams, if correct, were sufficient to show that, whatever he might have achieved he had certainly not obtained the substance commonly known as albumen. It is enough to point out that with the materials employed, the artificial product could not contain sulphur, which, at any rate up to the present, is regarded as an essential constituent of albumen.

The report of the International Congress of Applied Chemistry, given in the number of the *Chemiker Zeitung* (xxii. 644) just to hand, includes a short account of Dr. Lilienfeld's paper. Translated it runs:—

"Dr. Lilienfeld gave a very interesting account of the artificial synthesis of albuminous substances (*Erweis-körper*). It has been found possible to prepare pepton hydrochloride by the condensation of phenol and glyco-coll with phosphoric oxychloride; thus obtained, it gives all the reactions of the albuminoids. The lecturer experimentally demonstrated the preparation and properties of the new compound. By previous conversion into the

sulphate and decomposition of the latter, the free pepton can be obtained, and resembles, both in its chemical and physiological behaviour, the natural pepton from albumen. The analytical data corresponded with those given by natural pepton."

From this it is evident that Dr. Lilienfeld claims not the synthesis of albumen, but that of pepton, a digestion product of albumen, which, in spite of the statements of Henninger and others, does not seem so far to have been reconverted to its parent substance. In the absence of exact details, it is impossible to say how far the claim to the synthesis of pepton is justified, but it may be as well to recall previous work in the same direction.

Grimaux published in the *Comptes rendus*, about fourteen years ago, several papers on the formation of colloids from inorganic materials. Among others he obtained two: (1) by heating meta-amidobenzoic acid with phosphorus pentachloride, and (2) by the action of ammonia on solid aspartic anhydride heated at 170°. Although it was not to be expected that albumen would be obtained from such materials, it is remarkable how close was the resemblance between these colloids and the proteids when judged solely by their reactions.

A little later Schützenberger attempted the synthesis of proteids from the products of their decomposition. He had been engaged for some years on the study of the products of the hydrolytic decomposition of albumen by barium hydrate solution at varying temperatures. Among the substances obtained were various amido-acids of both the fatty and the aromatic series. He therefore dehydrated a mixture of these acids and urea with phosphoric anhydride, hoping thus to reverse the hydration process. Without giving details of the method employed, it is sufficient to say that he obtained a colloid which gave the reactions usually considered diagnostic of a proteid.

In 1897 Dr. J. W. Pickering (in continuation of a series of papers published in conjunction with Prof. Halliburton in the *Journal of Physiology*) contributed an interesting paper to the Royal Society's *Proceedings* (NATURE, 1897, 341), in which, besides confirming Grimaux's results, he added many valuable observations of his own. Among the most remarkable of these is the fact that the colloid obtained from aspartic anhydride is digested by pepsin-hydrochloric acid, and then gives the colour reactions for pepton, and, further, that it closely resembles the nucleo-proteids in its physiological action.

Dr. Pickering, moreover, greatly extended Grimaux's work, and prepared several new colloids, such as one from a mixture of tyrosine, biuret, and phosphorus pentachloride, a second from para-amidobenzoic acid and phosphorus pentachloride, and a third from alloxan, meta-amidobenzoic acid and phosphoric anhydride. These, together with several others, gave the reactions of the proteids, coagulated at definite temperatures, and produced intravascular coagulation of the blood. Still more noteworthy is the fact that according to the author they are optically active, like the natural proteids. Should this statement be confirmed, these would be the first optically active substances produced directly from inactive materials. As this feat has hitherto been regarded by chemists as improbable, if not impossible, these colloids are certainly worthy of closer investigation from this point of view.

Dr. Lilienfeld, too, has synthesised a substance giving the reactions of a proteid by condensation of a base which he called biuretdimethylene, with different amido-acids. It should, however, be noted that these workers, so far, have not claimed that the products obtained were actually proteids, but only that they bore a striking resemblance to them; and in this they were doubtless correct.

It is well known that the so-called "tests" applied to the detection of a proteid are purely empirical. Such

colour tests as Millon's, nitric acid, &c., have no real value; the colour developed *may* be due to the proteid molecule as a whole, but more probably to some decomposition product, and, as already mentioned, some colloids which bear no relation to actual proteids give reactions considered characteristic of these substances. Again, the peptons in their reactions strangely recall the alkaloids, especially in the precipitates they give with mercuric chloride, potassium periodide, phosphotungstic and phosphomolybdic acids, &c., while elementary analysis is of little value, as all the proteids give very similar figures, which in nowise indicate the striking differences met with in their physiological behaviour. When, in addition, it is remembered how extremely complex and mobile the proteid molecule must of necessity be, and the readiness with which changes in its constitution are brought about, something more than a few empirical colour and physiological tests will be required to convince chemists that pepton has been actually synthesised. Dr. Lilienfeld's results evidently need further investigation, and in the meantime the question raised by his announcement is distinctly one that calls for suspended judgment.

SIDNEY WILLIAMSON.

THE TOXICITY OF EEL-SERUM, AND FURTHER STUDIES ON IMMUNITY.

THE investigation of poisons, both bacterial and animal, has been pursued with such enthusiasm in so many parts of the world during the past decade, and the public have been brought into such close touch with some of the practical applications which have followed in the track of these investigations, that the term toxin and anti-toxin, unknown in the days of Dr. Johnson's colossal dictionary, may now without exaggeration be said to form part of the vocabulary of every well-ordered household.

But whilst the more striking beneficent results obtained in the study of immunity have become public property, so to speak, a mass of important and most interesting researches remain concealed from the layman's view, locked away, as far as he is concerned, in the pages of divers scientific journals.

Of such researches we may cite those which have relegated the blood-serum of eels to the category of poisons. This remarkable discovery was made as long ago as the year 1888 by A. Mosso,¹ of Turin, who found that the serum of eels, when subcutaneously and intravenously inoculated into animals produced fatal results, although it was quite harmless when introduced *per os*. Half a cubic centimetre of eel-serum inoculated into a dog weighing 14 lbs. killed the animal in seven minutes; and Mosso obtained similarly lethal results in the case of rabbits, guinea-pigs, frogs, and pigeons.

But little further attention appears to have been paid to this subject until Calmette,² in 1895, and Phisalix,³ in 1896, carried out further experiments on the toxic character of such serum from an immunising point of view, and this year we have had quite a crop of memoirs on eel-serum treated from various sides, and our information is consequently greatly extended concerning both the character of this poison and its antidote.

It appears that the toxic effect of this eel-serum varies according to the manner in which it is introduced into an animal, and the different quantities required to produce lethal subcutaneous, intravenous, and intraperitoneal inoculations respectively have been elaborately determined by Maglieri,⁴ who states that for every 2-lb. weight of

rabbit employed from '02 to '025 c.c. of serum is required in intravenous inoculations, '4 to '45 c.c. in subcutaneous inoculations, and '20 to '25 c.c. in intraperitoneal inoculations. Héricourt and Richet¹ mention that in their experiments '1 c.c. intravenously introduced was fixed as the lethal dose of serum for a rabbit weighing 4 lbs.

Wehrmann,² however, remarks that it is in reality very difficult to lay down a general law as to the exact quantity of this serum which will constitute a fatal dose, for it not only varies in toxic strength at different times of the year, but in eels of different origin; and it is, therefore, necessary to determine the toxic value of such serum each time a fresh supply is collected.

Before passing on to the experiments which have been carried out on modifying the lethal activity of this eel-serum, and on artificially protecting animals from its toxic action, we may refer to some interesting investigations made by Maglieri (*loc. cit.*) to ascertain whether such serum is endowed with any bactericidal properties. For this purpose tubes containing eel-serum were inoculated with colon bacilli (*B. coli communis*), cholera vibrios, and diphtheria bacilli respectively; after different intervals of time, varying from fifteen minutes up to twenty-four hours, gelatine and broth tubes were inoculated from all the serum-tubes. In every case a positive result was obtained; that is to say, growths of the three different microbes employed subsequently appeared in all the gelatine and broth tubes, indicating that, however lethal this eel-serum may be in regard to animal life, these minute vegetables—or, at any rate, the three varieties above mentioned—enjoy a natural immunity from its toxic action.

The quantity of blood which is procurable from even a large eel weighing about 5 lbs. is very small, never more than 25 cubic centimetres, and this only yields from 10 to 12 c.c. of serum, whilst in the case of vipers a relatively large quantity of blood is obtained. This eel-serum, according to Wehrmann, can be kept in a fit experimental condition for two weeks if stored over ice and in the dark, but Maglieri states that its toxicity declines gradually after the eighth day of its collection even when protected from light.

As regards the artificial modification of the lethal properties of eel-serum, U. Mosso,³ a brother of the Mosso already referred to, mentions, amongst other devices, that heating the serum to from 68° to 78° C. removed its toxic character. Phisalix (*loc. cit.*) also found that heating it to 58° C. for a quarter of an hour destroyed its toxicity, and that such heated serum was capable of endowing animals with immunity towards ordinary eel-serum, this immunity being, however, of a very transitory character. Wehrmann found that exposing it to this temperature for a quarter of an hour removed the greater portion of its toxic powers, and when animals were inoculated with serum thus treated, a somnolent state, sometimes accompanied by a depression of temperature, followed, but that they recovered their normal condition at the end of from two to three hours, having meanwhile acquired a certain degree of immunity from the effect of ordinary eel-serum inoculations, which was retained for three days. Maglieri found that preserving eel-serum at a constant temperature of only 37° C. for the space of twenty-four hours was sufficient to greatly modify its toxicity. Very interesting is the observation recorded by Wehrmann that by subcutaneously inoculating anti-venomous serum⁴ into eels the toxicity of their blood is considerably reduced. Thus an eel weighing about half a pound was inoculated with 5 cubic

¹ *Archives Italiennes de Biologie*, vol. x., 1888.

² "Venins, toxines et sérums antitoxiques" (*Annales de l'Institut Pasteur*, vol. ix., 1895).

³ *Comptes rendus de l'Académie des sciences*, 1896.

⁴ "Sull'azione tossica immunizzante e battericida del siero di sangue di anguilla." (*Annali di Igiene Sperimentale*, 1897.)

¹ *Comptes rendus de la Société de Biologie*, 1897.

² "Recherches sur les propriétés toxiques et antitoxiques du sang et de la bile des Anguilles et des Vipères" (*Annales de l'Institut Pasteur*, p. 810, 1897.)

³ *Archives Italiennes de Biologie*, 1889.

⁴ Serum derived from an animal rendered artificially immune to the poisonous action of snake-venom.